SCIENCE

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ON THE ABSENCE OF COW'S-MILK FROM JAPAN; ITS BENEFICIAL CONSEQUENCES.[1]

BY ALBERT S. ASHMEAD, M.D., NEW YORK.

ONE of the most striking features of that most curious of countries, Japan, is the singular scarcity of domestic animals. There you will never find the fields dotted with oxen or horses drawing the plough; for the Japanese are hardly acquainted with that time-honored tool and symbol of agriculture. Even to serve under the saddle does not come natural somehow to the Japanese horse; "a grudging, ungenerous animal, trying to human patience, with three movements (not by any mans to be confounded with paces), a drag, a roll, and a scramble."[2] Horses and cows are only seen in cities, and on the roads as pack-animals; there are no pastures sweet. Silence is here really a striking magnificent feature of the *rus beatum*. The cone-shaped mystic Fusiyama rises, dimly seen, in the midst of an awful quietness. No lowing herds wind o'er the lea; the barn-yard fowl's is almost a voice *clamantis in deserto*. He reminds the farmer, but only in the morning, that, even under these stagnant circumstances, time flies. Here and there, however, a dog howls; that is all.

The animal life of the land is set apart, concentrated, and taken care of, as in a kind of common preserve, a general park or reservation, in the interior of the land; where it browses and prances and bellows and reproduces itself, contaminating as little as possible that high type of eastern humanity which is now making ready for the baptism of western civilization.

But let me say, in passing, that what the European in Japanese fields misses, I believe, more than anything else, is

> "The music of those silver bells,
> Falling at intervals upon the ear,
> With cadence sweet."

I intend here only to speak of one of the consequences of this quaint absence of animal features, of something not poetical at all, but practical in the highest degree. The cow, in Japan, is not wanted for her milk; otherwise she would lift her voice more boldly in the landscape. Milk, being an animal product, falls under the general condemnation which excludes everything that has pertained to a living body from the alimentation of man. Now, it is true this latter rule has a strange exception, for the animals of the chase are eaten. Let us not shrug our shoulders at the apparent inconsistency; the Oriental mind understands itself. Thus it happens that, as Japan may not use cow's milk, the Japanese mothers are compelled by stress of circumstance to suckle their babes themselves; and these delicate dwarfs have become the most perfect, the most successful *Almæ Matres* of the world.

Artificial lactation is altogether unknown. The children are suckled until the sixth year, and you may hear them ask for the breast in a language as correct as that of adults. But it must be said that the mother's milk is not the only food of the little Japanese. River fish enter for a large part into their diet; after the first year some other elements of general alimentation are added to their bill of fare. But the mother's milk always remains the *plat de résistance*.

Nature and society have endowed this notable mother with some great and peculiar advantages. Here menstruation returns only a year and a half after birth. Moreover, rules dating back to time out of mind insure the young mother a long time of especial attention on the part of the husband and her whole household. The existence of the concubinate is also, strange as it may appear, a considerable relief to the Japanese matrons. All that must tell favorably on the health of the children. Even the infantile minds find themselves in a wholesome, pleasant medium. Nowhere are children so constantly, so lovingly taken care of. Japan, it has been said, is the very paradise of childhood. Nowhere are the adults so well qualified to enter into the nascent ideas of the infant, to play with him; for the nature of the Japanese contains an extraordinary proportion of simplicity and childishness.

The principal food of the mothers, besides the everlasting rice, is fish, shells, sea-weeds, and other products of the sea. No wine or beer enters into the diet of a lactating woman. The great reward which Japan reaps from this meritorious care of motherhood and childhood is the absence of rachitism. All observers have referred to the fact, and to the absence of rachitic pelves, which is the consequence of it; hardly any difficult confinement, and a very small percentage of deaths in child-birth. Now, I think I am not wrong in affirming that the chief and central source of these great sanitary blessings is the absence of cow's-milk.

It is a remarkable fact that Japan, which, according to Dr. Brush,[3] ought to be exempt from tuberculosis, is very far from being so. It is probably well known to you, that, according to this observer, tuberculosis passes from the cattle into the human organism. In Japan, this disease exists mainly in the upper classes where, evidently cow's milk has nothing to do with it, and, where it is easily explained by a systematic custom of close inter-marriage, a system of, according to our ideas, incestuous inbreeding, which has endured for many centuries; this is the same process by which the disease is developed in cattle, according to Dr. Brush. It seems, therefore, that there is no necessity of transmission, and that the human organism worked upon by the same causes will show the same effects. Strange to say, the mountaineers, who have the most intimate relation with the isolated Japanese cattle, on their breeding ground, are practically free from tuberculosis. There is also an historic fact which goes much against Dr. Brush's theory; the cattle were introduced into Japan, from China, in the third century, and tuberculosis is known to have existed there in that same high-bred class from times immemorial. The aristocratic disease, tuberculosis, was certainly communicated to the common people through a very extensive concubinate; and I am equally convinced that it was the milk of the mothers that preserved the lower orders from destruction.

Thus it would appear that the absence of cow's milk, though not a blessing in the sense of Dr. Brush, has had in another way an exceedingly beneficial influence on the general health of the race.

Racial immunities, or the natural resistance of a race to certain diseases, are at least partly transmitted by the mother's milk. It is thus, as I said, that this race is free from rachitism. And there is another privilege of the same kind transmitted through the milk to the suckling. The iodized sea-foods, more especially sea-weeds and the fats and oils of fishes, which have for so many centuries formed a considerable proportion of the

[1] Read in the Section of Diseases of Children, at the forty-third annual meeting of the American Medical Association, held at Detroit, Mich., June, 1892.

[2] Miss Bird, Unbeaten Tracks in Japan.

[3] See "The Relationship Existing between Human and Bovine Tuberculosis," by E. F. Brush, M.D., Mount Vernon, N. Y. Read before the New York Academy of Medicine April 18, 1889 (*N. Y. Med. Jour.*, June 15, 1889). Also "On the Coincident Geographical Distribution of Tuberculosis and Dairy Cattle," by E. F. Brush, M.D. Read before the Medical Society of the State of New York at its eighty-fourth annual meeting (*N. Y. Med. Jour.*, March 8, 1890). Also "Consanguineous Breeding in its Relations to Scrofula and Tuberculosis," by E. F. Brush, M.D. Read before the Society of Medical Jurisprudence and State Medicine, March 10, 1890 (*N. Y. Med. Jour.*, June 28, 1890).

diet of nursing mothers, have without doubt helped to build up the racial resistance to their national inheritance, syphilis and tuberculosis.

In the case of tuberculosis this resistance is so efficient that even the child of a tuberculous mother, fed on what might be supposed to be tuberculous milk until the sixth year, in the majority of cases remains unaffected. Now, if a tuberculous cow's milk transmits the disease to the human organism, why should not this tuberculous mother's milk transmit it? Even we do not object to the suckling done by our own tuberculous women, which indeed extends generally over but one year, yet their offspring, for the most part, are unaffected by the disease, at least in childhood; now it is more than likely that, if there were a contagion through milk, its effects would be apparent in the children. All these benefits would, of course, be cut off by the substitution of a foreign element to the natural means of transmission.

While I was in Japan, I conceived an idea quite satisfactory, at least to my own mind, of the manner in which the iodized food renders its great service to the Japanese race. It is generally supposed that the contagion of tuberculosis is communicated by the inhalation of particles of dried sputum disseminated in the air. It is my firm conviction that this is not so. I believe that these germs of disease are swallowed with the saliva, and alter the nutrition through the chyle and mesenteric glands. In an organism fed directly or indirectly by iodized substances, the poison meets and is neutralized by its own antidote. The Japanese mother, as by an instinct, never kisses her child on the lips. Indeed, the whole institution of kissing (except in the sexual act) is practically unknown in Japan. It is even formally condemned because the Japanese know that the kiss is the carrier of tuberculosis and syphilis. I have no doubt but that the caresses of the sick have added enormously to our own statistics of tuberculosis, and have caused much of the mischief which Dr. Brush would attribute to cow's milk.

I don't know whether the following has struck any other observer, or if I am the first to call attention to it. There is another, an occult and insidious danger which Japan escapes by letting cow's milk alone. If they drank it as we do, it is very probable that they would drink it as we do, *nolentes volentes*, mixed with a nobler fluid. Now, thanks to the rice plantations, the water of Japan is by no means the best of things; it is even the worst, for it is pregnant with typhoid germs, being continually polluted with human excrements and swarming with the brood of the distoma. Total abstinence from cold water, an inverted teetotalism, has been the salvation of Japan. Water is only drunk boiled with tea; the boiling kills the typhoid germs and the eggs of the distoma.

THE ETRUSCAN RITUAL BOOK.[1]

BY DANIEL G. BRINTON, M.D., LL D.

The discovery by Professor J. Krall of the fragments of an Etruscan book, written in the time of the Ptolomies, and preserved in the swathings of an Egyptian mummy, is an epochal event in archæology and cannot fail to excite the liveliest interest in learned circles. It has just been issued by the Vienna Academy of Sciences, and in a manner entirely satisfactory to the most exacting criticism. The mummy bands on which the inscription is written are reproduced photographically with the greatest care, and the judicious text and commentary by the editor are just what are needed, and no more than are needed, to place all the material for a thorough study of the document in the hands of the reader.

The circumstances of the discovery of the mummy and the inscription have been already briefly referred to in *Science*, Sept. 23. The first who noticed the writing was Professor Brugsch, in 1868; but he did not recognize it as Etruscan; nor did Captain Burton, who published a portion of it in 1879, although that versatile writer was the author of a book on Etruscan remains. Professor Krall, in February, 1891, was the first to make this remarkable identification.

[1] Die Etruskischen Mumienbinden des Acramer National-museums. Beschrieben und herausgegeben von Prof. J. Krall. Wien, 1892. In commission bei F. Tempsky.

The original condition of the book can be restored from its fragments. It was written on a piece of linen, at least 3.50 meters long, by 35–40 centimeters wide. The writing was in columns, so that when the linen was rolled, by unrolling it moderately, one such column, about 25 centimeters wide, could be commodiously read. The writing was done with a reed, and with ink made from carbon, like that which we know as "India ink," and which was usually employed in ancient Egypt. The letters were firm, clear and regular, plainly the work of a skilled caligrapher. The alphabet is that of a high class of Etruscan literature,—quite apart from those degenerated forms which are found in northern Italy. It is probable that the original roll was longer than the fragments indicate, and therefore that they only represent a fraction of the original work.

The linen on which the book is written is of Egyptian manufacture. But as at the date of its preparation Egypt supplied much of the Mediterranean world with the products of her looms, this does not prove that it was written in Alexandria. The question must be left undecided; but there is nothing else Egyptian about the scroll. The text contains no names of Egyptian gods or personages and no sign of foreign influence. It is wholly Etruscan in language, proper names, and general character, and at most may have been an Egyptian copy of an original brought from some Etrurian city.

The text offers twelve columns of about twenty-five lines to a column, six or seven words to a line. A number of the lines are incomplete, others are lost; but enough remains to offer an excellent apparatus to study the language. There are a number of repetitions, as of set phrases, and at the beginning of several paragraphs the Etruscan numerals are found, applied always to certain words of frequent recurrence. The names of various Etruscan divinities, as Nethunsl, Tinsin, Thesan, Usil, Uni, etc., are repeated, indicating clearly that this is some kind of a religious work. Professor Krall pronounces it a ritual to set forth the character and number of offerings (*Opferritual*). From certain arrangements noticeable in the text, I think rather it belongs to the class of works on divination, for which the Etruscan haruspices were so famous.

Something may be added to show the exceptional value of this find.

There is no greater mystery in the whole of European antiquity than that which surrounds the Etruscans. Niebuhr once said that he would willingly part with a large part of his fortune to be able to identify their ethnic relations. Up to the present time, this has been impossible. Not a single theory has been offered which has proved acceptable. Some of the ancients maintained that the primitive Etruscans came from Asia Minor; Virchow has written an article tracing them over the Alps toward the north-east; Dr. Isaac Taylor wrote a book to prove they were "Turanian;" Burton, in his "Etruscan Bologna," tore Taylor's hypothesis to tatters, but did not have better success with his own; and so on with an endless chain of attempted identifications.

The uniform tradition of the Etruscans themselves was that their ancestors came by sea to the shores of Italy, and landed first on the west coast, approximately about 1200–1300 B.C. Thence they extended over central and northern Italy as a conquering race, developing a remarkably high civilization, and finally succumbing to the Romans on the south and to the Celtic and other barbarous tribes on the north. They had settlements as far as the Rhetian Alps, and I have seen in the Museum of Chur, in Switzerland, tombstones with inscriptions in the Etruscan character from that locality. It is true, however, that this is not conclusive evidence; as it is quite certain that some inscriptions in this alphabet are not in the Etruscan language. Their alphabet was adopted by the Veneti, an Illyrian people, and also by the Celts, both of whom wrote in it their own tongues, or at least employed it in their mortuary inscriptions. As the matter now stands, in spite of our possessing over five thousand Etruscan inscriptions, some of considerable length and others bilingual, I do not hesitate to say that there is not a single word whose meaning we positively know.

A true Etruscan inscription was discovered some years ago on the island of Lemnos, in the Ægean Sea, showing that this sea-

faring people had extended their journeys, if not their colonies, to that comparatively remote quarter. This interesting relic has been ably worked up by Professor Pauli, who may be said to be at the head of living Etruscologists.

About the time that the Etruscans settled in Italy, a people of closely similar name, the Tursha, appear in Egyptian history as bold invaders and daring warriors. They are mentioned in the inscriptions of Meneptah II. and Ramses III., and by most writers are considered of the same stock as the Turseni, Tyrrheni, Tursci, or Etruscans. They were allies with the Libyans, and entered the Fayoom with these in the Ramesside period from the Libyan territory to the west. Professor Krall accepts this identification, but adds the cautious and just remark, that we have no positive knowledge of the language spoken by these Libyan neighbors of Egypt at the time mentioned. Of course, if they were the Tursha, and these were the Etruscans, we should see our way much more clearly.

CREMATION OF CHOLERA CORPSES.

BY ALBERT S. ASHMEAD, M.D., NEW YORK.

LET me add a few words to the article of mine, entitled "Cremation of Cholera Corpses," which you published Sept. 2.

I said in the New York *Tribune*, Sept. 22,[1] that religious prejudices should not interfere with the enforced cremation of cholera corpses.

This is what Professor Stillé writes to me about the subject: "In regard to cremation, I have no doubt of its being the proper way to dispose of the dead, and that it originated, as all sanitary laws did, not in divine command, but in human wisdom derived from experience.

"If the Egyptians had possessed fuel, I have no doubt they would have burned their dead, and that the Jews would have followed them in this as in most of their sanitary laws, e.g., circumcision, unclean meats, etc. Of course, with Greeks, Romans, and Christians the doctrine of the resurrection of the dead (most distinct, of course, in the last) led to the preservation of inhumation.

"There are many persons even now who believe in the literal resurrection of the actual body, albeit they are at a loss to give a reason for this popular belief. After all, I doubt if cremation of the dead will become usual. Superstition will hinder it among the ignorant, and tenderness among the refined."

There are in the history of the treatment of infectious and contagious diseases three periods.

1. There was a barbarous period when every, let us say, leper, was considered as outside of the pale of humanity, without any right to the sympathy of his fellow-men, only not killed because there is a law of the Decalogue against killing. The leper, as we have chosen him as the representative of this class of wretches, was condemned to solitude, absolute isolation; if he came by chance within hail of any fortunate healthy brother or sister, he had to ring a bell which he was obliged by law to always carry about him, in order to let them know that somebody was approaching who had no right to approach his fellow-being, and whose presence was an involuntary menace of death! These men were utter outcasts, enemies to be kept off as wild beasts are, completely neglected; when they were found dead, their carcasses were buried — that was the only duty which society performed in their behalf.

2. The second may be called the Mediæval-Christian period. Then something was done for them, in fact everything which those dark centuries knew how to do. *Misericordias* were formed, societies of St. Lazarus, etc. Asylums, hospitals were established. Of course, the greatest service the men of that time thought that they could render their unfortunate brethren was — prayers, the ceremonies of religion. For the ætiology was — visitation of God, punished sin, etc. In a time of epidemic the sanitary measures consisted in holy processions with banners flying, crosses, candles, holy-water; also relics, such as the seamless coat of Treves, a thousand ugly images of the Virgin meeting the traveller at every step. Have not we seen here in New York thousands kissing a bone?

3. The third period is the age of reason, the sanitary period, when superstition, ignorance, and fanaticism must be kept in check, brought to bay, utterly ignored, in every question of public health. We know now what we have to do; there is no excuse for not doing it. If, with the knowledge we have, we pander to the ridiculous pretentions of those who stupidly try to keep up the regimé of the Middle Ages, we are simply criminal.

[1] TO THE EDITOR OF THE *Tribune*: No more salutary measures have ever been taken against the spreading of cholera than the burning of the cholera corpses at Swinburne Island. It is evident that as long as the bacillus has not been entirely destroyed it will live to fight again. However deep it may be buried, at some time it will reach the surface again, get mixed with the water we drink, and cultivate itself in the human body. Why then should a measure so necessary for our safety be limited to such uncared for bodies as those who are found on vessels stationed at quarantine in the bay? The same danger threatens us from the bodies of those who die in the city. There is no use in saying that they will be buried in metallic coffins. Metal may keep the enemy in harmless seclusion for a longer time, but not forever. Moreover, metal renders the process of putrefaction slower, and keeps the bacillus which feeds on the corpse longer alive. There is probably no difference in regard to the danger arising from buried germs, whether the corpse be buried in wood or in iron. Therefore, it is evidently a duty of a board of health which cares truly for the public welfare to enforce cremation of all cholera corpses in the city as well as on the ships. Religious prejudices can really not interfere with that; the body reduced to ashes can resuscitate as well as the body buried, for it is clear that any corpse long before the general resurrection of the dead will be reduced to a condition entirely similar to that which cremation brings about. Or, if it is only the routine of the ignorant that stands in the way, it is the right and the duty of the educated and learned to impose by law and by force what is necessary to the welfare of the whole community. If we must bury our corpses, let us at least bury them in the most rational way possible. Wood decays, iron rusts or bursts, but earthenware jars are absolutely impermeable, and even indestructible. These have been used for more than a thousand years by the royalty and higher classes of Japan, and as we are, just now, teaching the Japanese so much, it is only fair that, when they are entirely in the right, and have given a great deal of thought to the matter, they should teach us something, too. They put vermillion with the cadaver; we might use bichloride of mercury.

SOME POINTS IN CHRONOLOGY.

BY R. W. MCFARLAND.

THE difficulties met with in chronology are best understood by those who have given most attention to the subject. In ancient times each nation was a law unto itself, touching the method of counting time or registering great events.

The Egyptians, several thousand years B.C., knew that the year was very nearly 365¼ days. They, however, dropped the fraction and retained only the whole number. It is said on good authority that this error of one-quarter was allowed to remain, so that by losing one-quarter of a day each year the seasons would slide forward around the whole heavens in 1461 years. By this slow motion of the seasons through the year, the festivals of the gods in like manner would be celebrated in all the seasons, to the end that all the gods should be honored equally and in exactly the same way.

The Roman calendar was amended by Julius Cæsar, 46 years B.C., with and by the aid of an Alexandrian astronomer. We use what is substantially the Roman calendar. It would not be proper in this place to enter into an explanation of the minutiæ of many points in doubt or in controversy. The immediate cause of Cæsar's reform was the vicious habit of the pontiffs in calling out or proclaiming the beginning of the months in such a way as to serve political ends or emergencies. Of course most people who are conversant with the derivation of words know that the word "calendar" is from the Latin *calare*, to call, or to proclaim. As a consequence of the reformation by Cæsar, the year 46 B.C. was made to consist of 445 days, and is sometimes known as the year of confusion. The year 45 B.C., the first of the reformed calendar, coincided in the main with the year 708 of the city of Rome. This is the Julian calendar which was followed in general by the Latin Empire, and was naturally adopted by the various nations after their incorporation into the Roman dominions. The old Egyptian year of 365¼ days was merely transferred to a more northern region, and into a far wider territory. It was not till long after the conversion of the Emperor Constantine to Christianity in the year 320, viz., in the early part of the sixth century, that the proposition was made to count the assumed date of

the birth of Christ, as the beginning of the era — the one now in common use by all Christian nations.

For ecclesiastical purposes the early Christians adopted in part, at least, the Jewish calendar, especially for the feast of Easter, the counterpart of the Jewish passover — the 14th of Abib, the first month of the year. "In the fourteenth day of the first month at even is the Lord's passover."— Leviticus xxiii., 5. But the fourteenth day did not generally fall on the Sabbath. Some churches celebrated Easter on the fourteenth, and some on the following Sunday. This caused some contention, and easily grew into a matter of supreme importance for the church. In the year 325 of our era, the council which convened at the city of Nicæa, beyond Constantinople, decided that the feast of Easter should be celebrated on Sunday, and that it should be the Sunday following the day of the full moon, which should occur on, or next after, the 21st of March. The intention was to fix the time of Easter as nearly as a movable feast could be fixed. The Jewish year was luni-solar — twelve months for one year, thirteen for the next.

Early in the fifteenth century the ecclesiastics noticed that the equinox was slipping away from the 21st of March. The question was discussed more or less for nearly two hundred years before final action was taken. In 1582 the equinox occurred on the 11th of March instead of the 21st, as at the time of the Council of Nicæa, in 325. Pope Gregory XIII., with the aid of able coadjutors, reformed the Julian calendar. His object was to prevent in the future such diversity of days in celebrating the same feast. The change made by Gregory consisted chiefly of two points: 1, The skipping of ten days in order to bring the equinox back to the 21st of March; and 2, To arrange an order of leap years which should prevent a like divergence thereafter. The omitted days were the ten following the 4th of October, 1582. The day which in the ordinary course of events would have been the 5th was reckoned as the 15th of October, new style. The Julian calendar, with every fourth year a leap year, is old style. Gregory excepted the centesimal years, decreeing that only those which are divisible by 400 should be called leap years. The year 1600 being divisible by 4 and by 400 was a leap year in both styles. Wherefore the difference between the two styles continued ten days for a century after 1600, viz., till midnight of the 28th of February, 1700. In new style, 1700, not being divisible by 400, was a common year, and the day following the 28th of February was March 1. But in countries which still adhered to the old style, 1700, being divisible by 4, was a leap year; so the day following the 28th of February was the 29th. Here there began a difference of eleven days between the styles. A like case occurred on the 28th of February in 1800, and the difference became twelve days, and will so continue till February 28, 1900; after which for 200 years the difference will be thirteen days. Russia still adheres to the Julian calendar, and the 12th of October, 1892, in that country will be the 24th in this.

The change of style by Gregory looked solely to the future, in order to prevent unseemly changes in the time or date of church festivals. It did not disturb the past at all, and was not intended to do so. As a proof of this, it may be stated that no date previous to October 4, 1582, old style, was ever changed by Gregory or any of his successors, or by any body of learned men, or of unlearned men; that no writer of history or of chronology in any European nation has changed or attempted to change such dates from old to new style. The discovery of America was on Friday, October 12, 1492, old style. It is so written "always and everywhere and by all."

It was reserved for the American Congress of 1892, instigated by a committee of some ill-informed society, to depart from established and uniform custom, and to declare that the 21st of October, 1892, should be celebrated as the 400th anniversary of the discovery. It is a "consummation devoutly to be wished" that this hasty and ill-advised action of Congress may die a speedy death, and that after this year it may never again be thought of or regarded in any way.

The present Pope, in his announcement concerning "Columbus Day," utterly ignores this act of Congress. He says, according to current reports in the daily press, that on the twelfth of October or on the following Sunday (the 16th) appropriate services will be had in commemoration of the great discovery. It is to hoped that some friend will call his attention to the unadulterated wisdom displayed on this side of the Atlantic, regardless of the "effete monarchies" of Europe.

England adhered to the Julian calendar till about the first of September, 1752. To be specific, the order of Parliament was that the day following the second of September of that year should be called the fourteenth, and that the year which previously began on March 25 should begin on January 1, 1752, to conform to the Gregorian calendar. Macaulay, Hume, Robertson, and all other historians who have written in the English language of events in English history, give the dates in old style up to the year 1752.

In the colonies on this continent, planted by the French, Dutch, Spanish, and English, each followed the custom of the mother country, some using old and some new style. After the Revolutionary War Ramsay's Life of Washington was written. In it Washington's birth is given in old style only, viz., February 11, 1731,— conforming to the English custom of leaving unchanged all dates before the change of style. But "necessity knows no law;" so the conflicting dates of the various colonies were assimilated by all being made new style, for events occurring on this continent.

Such is a brief account of some points in chronology, which account may be of interest to many and may stir up some to a more careful study of a much neglected subject.

Oxford, Ohio, September, 1892.

SOME THOUGHTS ON THE PHYLOGENY OF THE MOLE CRICKET.

BY E. W. DORAN, PH.D., COLLEGE PARK, MD.

I HAVE recently been able to work out to some extent the life-history of the Northern Mole Cricket, *Gryllotalpa borealis*. The various stages of the insect seem not to have been studied extensively, or described, before. I have made some observations of interest which I have not seen recorded elsewhere, and which seem to indicate the course of development in this species. I am led to believe that formerly the insect lived upon the surface of the ground, or in natural hiding places, very much like our common field cricket, instead of burrowing into the earth, and passing all its existence under ground.

My first reason for supposing a change of habit has taken place is based upon the fact that the larva, before the first moult, is able to jump like the field and house-cricket. (Larvæ but little over a fourth of an inch long were seen to jump five or six inches in the breeding-jars.) They are otherwise very active and brisk in their movements. After this stage the insect cannot jump at all, and is very clumsy. It can run rather rapidly backward or forward in its burrow, or upon a level surface, but has very awkward movements upon an uneven surface. The abdomen is long and heavy, especially in the pupa and imago.

Now this would indicate that originally the mole cricket had the power of jumping like most other orthoptera, and all other *Gryllidæ*, I think, and lived upon the surface of the ground, perhaps hiding in crevices, or under rubbish, like the common cricket. But having taken to the burrowing habit, and no longer finding the necessity for exercising its power of leaping, it gradually lost that power, until it appears only in the early part of the larval stage.

My second reason for this conclusion is based upon the habit the mole cricket has of defending itself in the burrow by ejecting posteriorly a creamy, viscid substance in large quantities, which rapidly thickens after exposure to the atmosphere. This fluid seems also to have peculiar chemical properties. In this way it is able to protect itself from almost any foe which may attack it from behind, and it fights viciously if attacked in front. Now, the larva before the first moult does not have the power of ejecting this substance, and this would possibly indicate that in a previous stage of its development the mature insect was not so armed, for the young larva certainly needs protection as much as

in later life. This method of defence would not be so effectual upon the surface of the ground where its enemy could attack it from any source instead of directly behind or before, because with its unwieldy body it would not be able to eject the substance in any desired direction suddenly. This, then, appears to be a habit acquired by the insect since it has taken to its underground life; for it is hardly probable that it would be provided both with the habit of making long leaps to escape from its enemies, and at the same time to eject in large quantities this protective fluid.

There are some rather serious objections to this theory of changed conditions and habits. First may be mentioned the unusual development of the tarsus, fitting it for its underground life and burrowing propensities; but it is not unreasonable to suppose that the front legs were developed gradually in conformity with its changing habits. And it is perhaps true that if the insect lived upon the ground, it occasionally burrowed for roots, or for shelter, and originally had an unusual development of the tarsus. Its carnivorous habits may have been acquired in consequence of its frequent contact with earth-worms, when other food was scarce, as there are many other insects which normally feed upon vegetable food, that will resort to animal food, devouring even their own kind, as in the mole cricket, when pressed by hunger.

Second, an observation made by Westwood and others in Europe upon *G. vulgaris* would seem to weaken my argument regarding the development of the insect. It is stated that the larvæ of the European species, before the first moult, live together in one burrow, with the mother cricket, but scatter after this moult. I have seen the very young larvæ of our species only in confinement, and cannot say whether in the natural state they would scatter before this time or not. They run about in the breeding-cage more before the first moult than afterward. However, I think it probable that the mother cricket feeds the young, at this early stage, as she exercises great solicitude for them apparently, in other matters, or in time of danger. I have several times seen the mother take the young in her mouth when disturbed, as a cat does her kitten, and carry them to places of safety. She will also carry her eggs to a new burrow when they have been discovered, as I have several times observed. Hence it seems probable the young larvæ live together under the protection of the mother cricket, and would have but little need of a protective ejection. But the jumping habit which is chiefly useful in escaping from their enemies, being confined only to the early larval stage, presents a stronger argument for changed conditions. I may say, however, that neither of these are presented as conclusive arguments but rather mere suppositions or suggestions, to be followed up by other observations.

THE SCIENCE OF SMELLING.

BY PROF. DE VOLSON WOOD, STEVENS INSTITUTE, HOBOKEN, N. J.

THE greater part of the science of seeing is contained in the science of optics, and this is founded upon the theory of undulations of the ether and the way in which they are modified by the media through which they pass. The form of the surface, whether plane or curved, as well as the density of the medium produces marked effects.

Similarly, the greater part of the science of hearing is contained in the science of acoustics, and this also treats of undulations, or waves propagated in air or other gases. It is not believed in either case that solid particles pass from a source to the sensitive nerves to produce the particular sensation.

Why should there not be a science of smelling? The principal part of such a science would consist of an investigation of the mechanical properties of odors, and might briefly be called "Odorology." Is it not highly probable that odors are also propagated by undulations of an ether? And yet we are familiar with the statements made by writers, such as "A grain of musk will keep a room scented for many years. During the whole of the time it must be slowly evaporating, giving out its particles to the currents of air to be wafted presently out of doors; yet in all this time the musk seems to lose but little of its weight." "The acute sense of smell of the dog is well known; for he can detect the track of his master long after the tracks have been made, which shows that some slight characteristic matter is left at each footfall."

Those who thus speak impart the idea that odor is material. I prefer to think of it as a property of matter, which produces its own peculiar undulations; and that the sensation of odor is produced by these undulations in the olfactories. Musk retains this property for a very long time, while some bodies lose it rapidly. The man may leave some characteristic matter on the ground at each footstep, but it is not necessary that particles of that matter shall pass from the ground into the nose of the dog in order that he may track his master. It is only necessary that that matter shall possess the property of sending forth certain undulations. Indeed, it is not difficult to conceive that the ground itself has imparted to it the property of sending forth the desired undulations.

These facts being assumed, investigations might be made to determine the velocity with which odors are propagated, and whether they are subject to reflection, refraction, and interference, and other properties common to sound and light; also whether the different odors are due to different wave-lengths, and if the strength and intensity of the odor is due to the amplitude of the wave, as in light and sound. The physiological qualities of the olfactories by which they enable one to detect odors of different qualities and intensities furnish a field for the most delicate and refined investigation.

NOTES AND NEWS.

PERSONS who are very susceptible to the effects of gnat-bites state that the irritation seems to return on the third day, and in those who have exceptionally sensitive skins again on the sixth day. Thus the effects of gnat-bites, or rather of the poison which they instil into our blood, have a certain analogy with the symptoms of intermittent fever. This need, perhaps, scarcely surprise us if we recollect from what materials the juices of the gnat have been elaborated.

— Herbert M. Thompson's "The Theory of Wages and its Application to the Eight-Hours Question," published by Macmillan & Co., is a timely contribution to the discussion of this vital question of the present industrial crisis.

— Lovers of birds and flowers will delight in Miss Yonge's new book announced by Macmillan & Co. Under the title of "An Old Woman's Outlook" she describes out-door life in England as she has watched it for so many years. The chapters are so arranged that each shall cover its special month.

— "Round London, Down-East, and Up-West" is the self-explaining title of a new book by Montagu Williams, barrister, author of "Leaves From a Life," etc. It will be issued by the Macmillans.

— D. Appleton & Co. will shortly add to their list of Good Books for Young Readers "Along the Florida Reef," by C. F. Holder, which is a story of camping and fishing adventures in company with a naturalist in Florida. The author combines entertainment and instruction, and his book is filled with illustrations which will be prized by every young reader who has ever visited the seashore, or cares for information regarding fishes, shells, and the various forms of marine life. The same firm will publish immediately "The Story of Columbus," by Elizabeth Eggleston Seelye, edited by Dr. Edward Eggleston, with nearly a hundred illustrations by Allegra Eggleston. This book is the result of extensive investigations which have been carefully verified by Dr. Eggleston. While the book contains all the results of modern inquiry offered in the bulkiest biographies, the story is here condensed and the material selected with a view to an always interesting narrative. To a considerable extent the plan of both text and illustrations is like that of Eggleston's "Household History of the United States." "The Story of Columbus" will be the first volume in a series to be called Delights of History, which will be prepared by the same author, artist, and editor.

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VON DEN LICHTSTRAHLEN KLEINSTER WELLENLÄNGE.

VON VICTOR SCHUMANN IN LEIPZIG.

Lange Zeit galten den Wellenlängen zweier Linien des Aluminiumspectrums als die kleinsten. Nach den Messungen A. Cornus betrugen die Längen dieser Linien in Angströmeinheiten (1 Angströmeinheit = 0.0000001 Millimeter) ausgedrückt, 1860 und 1852 AE. Für beide Linien ist, wie schon für die ganze Spectralregion des Ultravioletten, das menschliche Auge vollständig unempfindlich. Nur sehr wenigen ist es vergönnt, das Ultraviolett durchs Ocular eines hinreichend lichtdurchlässigen Spectralapparats ebenso deutlich wahrnehmen zu können, wie die minderabgelenkte Region des sichtbaren Spectrums, das, wie allgemein bekannt, von jedem gesunden Auge vollkräftig empfunden wird. Das ultraviolette Licht lässt sich nur auf einen Umwege sichtbar machen; entweder projicirt man es auf einen fluorescirenden Schirm oder fixirt es mit Hilfe der Photographie auf einer lichtempfindlichen Platte.

Das Fluorescenzspectrum kann man direct oder durch eine Lupe betrachten; in beiden Fällen lässt es aber an Klarheit und Schärfe viel zu wünschen übrig. In früheren Jahren, wo die photographische Platte dem nassen Verfahren angehörte und die moderne Trockenplatte noch nicht bekannt war, hat man sich vielfach des Fluorescenzspectrums bedient, wenn es sich um Versuche mit ultravioletten Strahlen handelte. Gegenwärtig, wo die Bromsilbergelatineplatte der photographischen Beobachtung so ausserordentliche Vortheile gawährt, denkt wohl niemand mehr an die Verwendung des unvollkommenen Fluorescenzspectrums.

Die photographische Beobachtung hat die oculare aus dem Ultravioletten vollständig verdrängt. Wer beide Methoden geübt hat, wird mir beipflichten, wenn ich sage: das Fluorescenzspectrum ist viel zu roh, als dass es der exacten Spectroskopie der Gegenwart noch gewachsen wäre.

Die moderne Trockenplatte ist gegen die ultravioletten Strahlen ungemein empfindlich und diese hohe Empfindlichkeit kommt der Spectralwissenschaft ausserordentlich zu statten. Zeigt doch die moderne Trockenplatte allen lichtquellen elektrischen Ursprungs gegenüber ihre höchste Empfindlichkeit nicht etwa im sichtbaren Spectrum, sondern weitab davon im Ultraviolett.

Photographirt man das Spectrum irgend eines Metallfunkens, so entwickelt sich jederzeit zuerst das ultraviolett, und erst, wenn man länger belichtet, tritt das sichtbare Spectrum hervor. Es ist aber keineswegs das ganze ultraviolette Licht, was dem sichtbaren voraneilt. Nur ein Theil davon zeichnet sich durch photographische Ueberlegenheit aus. Alles Licht, das jenseits der Kadmiumlinie No. 24 wirkt, braucht zu seiner Aufnahme beträchtlich längere Belichtungszeit. Die Empfindlichkeit der Platte nimmt von dieser Linie — ihre Wellenlänge beträgt 2306 AE — an mit der Brechbarkeit der Strahlen sichtbar ab, und sinkt, bei Anwendung grosser Aufnahmeapparate, in der Gegend der Wellenlänge 2000 sogar auf Null hinunter. 2000 AE dürfte demnach annähernd die kleinste Wellenlänge sein, die sich mit den gegenwärtig am meisten im Gebrauch befindlichen grossen Gitterapparaten noch beobachten lässt.

Versucht man diese Wirkungsgrenze mit einem kleinen Apparat zu photographiren, dann erweitert sich das Beobachtungsgebiet um eine ansehnliche Strecke, und die gewöhnliche Bromsilbergelatine erweist sich, bei hinreichend kurzer Focalweite und gehöriger Lichtdurchlässigkeit des optischen Körpers, sogar bis zur Wellenlänge 1820 geeignet. Dieses relative Grenzgebiet kleinster Wellenlänge gehört nach umfassenden Versuchen, die ich im Jahre 1890 anstellte, einem Apparat an, dessen Focalweite 180 millimeters (Fraunhoferlinie D.) betraegt.

Der Umstand, dass das photographischwirksame Spectrum um so weiter ins Ultraviolett hinausläuft, je kürzer die Brennweite ist, besagt deutlich, dass der Ort der photographischen Wirkungsgrenze eine Function der Dicke der Luftschicht ist, die die Strahlen auf ihrem Wege zur photographischen Platte zu durchsetzen haben. Versucht man nun, von dieser Thatsache ausgehend, die Luftschicht noch weiter zu vermindern, dann bemerkt man zwar, dass sich die photographische Wirkungsgrenze noch um einige Linien kleinerer Wellenlänge entfernt, allein der Längenzuwachs des Wirkungsbandes ist so unbedeutend, dass der Erfolg die Mühen und Kosten der Herstellung eines derartigen kleinen Spectrographen nicht lohnt. Es gewinnt sonach den Anschein, als habe man hiermit das wahre Grenzgebiet der wahrnehmbaren Lichtstrahlen kleinster Wellenlänge erreicht. Bestärkt wird man in solcher Annahme noch durch die Thatsache, dass das Fluorescenzspectrum ungleich früher, bei Wellenlänge 1852 verlischt, und demzufolge zur Beobachtung aller stärkerabgelenkten Strahlen ganz ungeeignet ist. Stände uns nicht die photographische Platte, sondern nur die fluorescirende Platte zu Gebote, so würde die kleinste Lichtwelle, die wir noch wahrnehmen könnten, nur das Längenmass von 1852 AE haben. Man sieht hieraus, dass beide Grenzwerthe nur eine ganz relative Giltigkeit haben. Aehnlich der fluorescirenden Substanz, die schon von Wellenlänge 1852 an nicht mehr leuchtet, könnte ja möglicherweise auch der photographischen Platte die Fähigkeit fehlen, von Allen Strahlen, deren Wellenlänge kleiner als 1820 ist, einen entwicklungsfähigen Eindruck anzunehmen. Diese Ueberlegung leitete mich, als ich vor nunmehr zwei Jahren eingehende Versuche mit Strahlen des brechbarsten Ultravioletten anstellte, und nicht ohne Erfolg. Es ergab sich hierbei, dass es nur der Mangel an Empfindlichkeit der damals angewandten lichtempfindlichen Platte, keineswegs ungenügende Energie der Lichtstrahlen war, die meine Versuche jenseits 1820 zu keinem befriedigenden Resultate kommen liess. Ich gewahrte ferner, dass die Strahlen schon in der das lichtempfindliche Silberkorn umschliesenden Gelatinehülle erstickten, ehe sie zur Einleitung des Zerfalls dieses Kornes gelangten. Die Gelatine des Plattenüberzugs bildete sonach die Ursache meiner photographischen Misserfolge im äussersten Ultraviolett. Die Kenntniss dieser wichtigen Thatsache führte mich zur Präparation einer neuen Platte, die sich in der Folge zur Photographie aller Strahlen jenseits Wellenlänge 2260 besser eignete, als die vorher benutzte Gelatineplatte.

Die neue Platte verhält sich den Lichtstrahlen gegenüber durchweg ganz anders wie die Gelatineplatte. Wenig empfindlich gegen alle Strahlen des sichtbaren Spectrums und der wenigerabgelenkten Strahlen des Ultravioletten, wächst ihre Erregbarkeit von 2260 an bis in die Gegend von 1860. Bei 1860 scheint sie, wenigstens allen elektrischen Lichtquellen gegenüber—andere Lichtquellen erzeugen niemals so starkabgelenkte Strahlen — die höchste Empfänglichkeit für Lichteindrücke zu besitzen. Weiter nach der brechbarern Seite hin sinkt ihre Empfindlichkeit etwas, doch bleibt die Wellenlänge 1820, bei der die Gelatineplatte aufhört empfindlich zu sein, ohne allen hemmenden Eindruck auf sie. Kräftig und klar gezeichnet, gibt sie das spectrale Wirkungsband auch jenseits 1820. Arbeitet der Spectralapparat mit einem Prisma, dann scheint es, als wollten die Lichtmassen, die diesen,

für das menschliche Auge in ewige Nacht gehüllten Strahlenbereiche entquellen, gar kein Ende nehmen. Mit jeder folgenden Region, die man zur Aufnahme einstellt, meint man das Endgebiet der kleinsten Lichtwellen zu erreichen. Aber es ist fast, als flöhe die kleinste Welle, die überhaupt noch photographisch zu fesseln ist, um so behender ins fernste Ultraviolett hinaus, je näher ihr die Fessel der lichtempfindlichen Platte rückt.

Schon jetzt weist meine neue Platte jenseits 185λ ein Spectrumband auf, dass das gesammte Wirkungsgebiet der Bromsilbergelatine um mehr als das dreifache an Länge übertrifft, und gleichwohl lässt auch die letzte meiner Aufnahmen noch der Hoffnung Raum, dass jenseits des Randes ihrer Platte noch photographisch wirksames Licht existirt. Vorläufig gehört aber diese letzte Aufnahme, ohngeachtet solch' günstiger Aussicht, doch demjenigen Gebiete an, das ich gegenwärtig als die Grenze der kleinsten Lichtwellen bezeichnen muss. Die Photographie des Nachbargebietes hiervon stösst zur Zeit,— aus Gründen, deren Erörterung hier zu weit führen würde,—auf Hindernisse, die sich, sofern es überhaupt möglich ist, nicht ohne grosse Anstrengung werden beseitigen lassen.

Fragt man nun nach dem Masse der kleinsten Lichtwelle meiner Ultraviolettaufnahmen, dann muss ich leider bekennen, dass mir im Augenblick eine bestimmte Antwort hierauf nicht möglich ist. Wellenlängen lassen sich im luftleeren Raume, an den meine Aufnahmen gebunden sind, nicht so leicht ermitteln wie in der Luft, und die geplanten Messungen der Wellenlingen des äussersten Ultraviolett haben darum auch besonderer Vorbereitungen bedurft. War es doch überhaupt zweifelhaft, ob sich die übliche Methode der Messung der Wellenlängen auf den in Rede stehenden Lichtbereich werde anwenden lassen. Meine Vorversuche hierzu gehen zur Zeit ihrem Abschluss entgegen, und die mir vorliegenden Resultate berechtigen zu den besten Hoffnungen. Unter solchen Umständen kann ich das Mass der kleinsten Lichtwelle, die meine Aufnahmen aufweisen, vorläufig nur schätzungsweise und mit Vorbehalt nennen. Es dürfte dieses Mass 1000 AE nicht überschreiten, ja eher kann es um ein gutes Theil kürzer sein.

Der Wellenlänge 1000 entspricht eine ausserordentlich hohe Schwingungszahl des Lichtäthers. Während die brechbarsten Sonnenstrahlen wenig mehr als 1000 Billionen Schwingungen in der Secunde ausführen, schwingt ein Strahl von der Wellenlänge 1000 in derselben Zeit dreibillionenmal. Mit Schwingungszahlen so enormer Höhe hat der Spectroskopiker bisher noch nie zu rechnen gehabt, und gleichwohl liegt es nicht ausser dem Bereich der Möglichkeit, dass wir über kurz oder lang die Wirkungen des Lichtaethers bis in die nächste Nähe der Wellenlänge Null verfolgen werden, wo der ungeheuren Anzahl seiner Schwingungen kaum noch der Massstab des Endlichen gewachsen ist.

THE SOUNDS OF R.[1]

BY ALEX. MELVILLE BELL.

THERE seems to be special need for a better understanding of the sounds of R. No element of speech is so variously pronounced —in dialects and by individuals. The fundamental organic action from which all the varieties are derived is a frictional emission of breath or of voice between two surfaces in the breath channel. Thus we may make an R in the throat,—producing the effect which, when prolonged, is called a groan; or in the guttural passage, between the back of the tongue and the soft palate—a mode which is dialectically common in many countries. A less definite variety is formed between the arched top of the tongue and the roof of the mouth. This is common in the United States. Another—and the normal English form of R—is produced between the point of the tongue and the upper gum. This is sometimes modified by inversion of the tongue within the palatal arch, or by addition of guttural or of labial contraction. The point-tongue R is also varied by advancement of the tongue to or between the teeth. In a common English affectation the seat of R is transferred fron the tongue to the lips, so that R has the sound of W. Of these varieties one may be characteristic of a dialect, another a mere individuality, but they are all effects of only one organic action performed at different parts of the mouth.

Another series of R's results from a trilling or rattling organic vibration instead of a mere friction of the breath or voice. Thus a trill of the epiglottis is heard as one form of R; a trill of the uvula is another and very common one; and a trill of the point of the tongue is the regular form of R in North Britain and Ireland. The Spanish R has a more prolonged rattle of the same kind. The trill has often the effect of a syllable; as in Scotch and Irish, where it converts the grammatical monosyllables *world, harm, mourn*, etc., into the phonetic dissyllables wor-rld, har-rm, etc.

These trills involve a strong pressure of breath and a harshness of phonetic effect, in contrast to which is a form of R of simple vowel quality, without friction or vibration; as in (ə)ise and (ə)ound, for *rise* and *round*.

A similar vocalic effect is also heard for R wherever it is not followed by a vowel; as in *here, care, fire, store, tour, are, war, term, first*, etc. The syllable-like quality of this sound is distinctly felt after the close vowel ē, and less distinctly after open vowels, because their mouth-cavities differ so little from that of R.

In Early English R was always trilled, as it continues to be in Scotland, where most of the characteristics of Early English are still prevalent. But in modern English the trills have been softened away wherever R follows a vowel, until little is left of the R but its vowel quality. We are accustomed to the entire omission of R in negro speech, where *do* and *sto* are all that we hear for *door* and *store;* but in educated utterance there is some phonetic effect left in R even where it is least manifest. Such delicate shades of sound are the distinguishing marks of refinement in pronunciation, and they should be carefully preserved by teachers and by writers on phonetics.

In a book recently published in England the learner is taught that R is silent in such words as *farm, serve, lord, prayer, weird*, etc. Had the statement been that the sound of consonant-R is not heard in these words it would have been correct, but the R is certainly not "silent;" it has a phonetic effect of its own, soft and vowel-like, but a quality wanting which the words would not have their characteristic pronunciation.

That there may be no mistake as to the teaching in the work referred to, the reader is specifically told that the words *arms* and *lord* are exactly the same to the ear as the words *alms* and *laud*. Now what is the sound of R which baffles the discrimination of this writer? Let us magnify it, as in a microscope, by prolonging the elementary sounds. First let us put "alms" and "laud" under the microscope:—

a----lms; lau----d.

Here there is no R; the vowel remains unchanged until stopped sharply by the succeeding consonant. Now put "arms" and "lord" under the microscope:—

a----(ə)rms; lo----(ə)rd.

Here between the vowel and the m or d there is interposed a gliding connective sound, so that the vowel is not stopped sharply by the consonant, but its quality is gradually changed by a lift of the tongue, verging towards but not quite reaching the position for R. This is all the sound that R has, in modern English, before any consonant or when final in a word. But it is something more than nothing; and something that is essential to the correct utterance of any word containing R before a consonant.

Among the sounds of R may be reckoned the influence of R upon other sounds. The mouth-cavity for R being very large, any closer vowel preceding R is, as it were, stretched at the point of junction, so as to assimilate with R. Thus a pure ē is with difficulty pronounced before R; a pure ā is never, in Anglican speech, heard before R, but a is "stretched" to eh, as in *air, chair*. So, too, o and oo before R have a more open than their usual formation, as in old—ore; pool—poor.

These widened sounds of o and oo are distinctly different from the sound of aw; yet in the book before referred to the words *shore* and *drawer* are said to have the same vowel; and the words

[1] Paper read before the Phonetic Section of the Modern Language Association, December, 1891.

your and *yore* are classed as identical in sound. Your, shore, and drawer are thus "phoneticised" into yawer, shawer, and drawer. These words are, indeed, often so pronounced in dialectic speech; but the science of phonetics must be retrograding instead of advancing when, in an "Introductory Science Text-Book," such differences can be ignored, and such mere negligences cited as examples of correct usage.

All short vowels stop sharply on consonant-R, as on other consonants, as in *parrot, very, spirit, sorry, hurry;* but long vowels take on the connective glide even before consonant-R, as in *weary, fairy, wiry, gory, fury.* Thus wea(ə)ry, fai(ə)ry, wi(ə)ry, etc.

The vowel quality inherent in the mouth-cavity of R is that of er-ir in *her, sir.* Consequently, in such words as *firm, yearn,* the r has the effect of lengthening the syllable by making it contain two sounds of the same vowel. Let us put the words under the microscope: —

fl----(ə)rm; yea----(ə)rn.

Test this further by analyzing the syllable "word." If the r were "silent," the vowel would be sharply stopped by the consonant d. Thus, wo----d; but the true pronunciation of this syllable interposes a glide between the vowel and the d. Thus, wo----(ə)rd.

In forming this smooth transitional r the tongue is slightly lifted from the bed of the jaw; therefore when a vowel follows the r, the consonant sound of the letter is also developed; as in fe'ring = fear-ring There is a tendency among many speakers to finish all open vowels with this lift of the tongue, so that the consonant r is inadvertently interpolated between two words, as in "Pennsylvania-r-Avenue, "I saw-r-it all."

Nice distinctions — like those which are the subject of this paper — are of no importance where mere intelligibility is concerned; for example, in the speech of the deaf. In such cases, the widest differences may be disregarded, so long as the words are understood. But in the study of phonetics, the most minute varieties require to be distinguished, because what in one case may be a distinction with but little difference, may in another become a very shibboleth.

I make no apology for introducing so small a topic to your attention. In a practical subject nothing is too small to be carefully investigated. The whole organism of speech is but small, and the differences of organic action from which the greatest elementary distinctions result are, in actual measurement, exceedingly small.

The sounds of R, with all their differences — rough, smooth guttural, lingual, labial, definite, indefinite — are only one in kind; and we must recognize them in their faintest as well as in their most obtrusive forms.

ON THE SECULAR MOTION OF A FREE MAGNETIC NEEDLE.[1]

BY L. A. BAUER.

A MAGNETIC needle suspended so as to move freely in all directions will set itself tangent to the lines of terrestrial magnetic force. At any particular epoch it will have a definite direction. It will make a definite angle with the meridian, which, measured in the horizontal plane, is known as its declination, also a definite angle with this plane, which, measured in the vertical plane, is termed its inclination or dip. About this mean position of equilibrium a variety of small periodic variations take place, accompanied at times by fitful or irregular ones, which occasionally become quite respectable. Concerning this we shall have nothing to say. But the needle undergoes another, and by far the largest excursion, requiring centuries for its fulfilment. Since its discovery in 1634 by Gellibrand, as exhibited in the secular variation of one of its co-ordinate angles, the declination, it has been the cause of no end of fruitless speculation. It has engaged some of the best minds and given rise to most ingenious theories, but the riddle is still unsolved.

As the needle assumes different positions for different epochs, it gradually sweeps out in space a cone, whose vertex is the centre of gravity of the needle. Or, if you describe a sphere having as a centre the centre of gravity of the needle, and prolong the axis of the needle until it intersects the sphere, the successive intersections will form some tortuous curve. The geometric nature of this cone, or of this tortuous curve, remains to be investigated. A preliminary analytical attempt was made by Quetelet in 1877. He used fifty years of continuous observations of declinations and dip made at Brussels, and found that a cone of revolution would best fit his observations, the period of a complete revolution being 513 years.[2] Mr. Schott made a graphical attempt for an average New England station, using about fifty years of observation. The scantiness of his material prevented him from making any safe deduction as to the course of the needle.[3]

To our knowledge, however, no attempt has as yet been made for the long series of observation which we possess at quite a number of stations. The usual custom is to discuss *separately* the secular variation of the different magnetic elements, as though they were *different* effects of forces acting, instead of *component* effects. We believe that this, in some measure, is the reason that with 100-300 years of observation no greater headway has been made in the conception of the requirements of the secular-variation problem.

With the view presented of the problem, some of the interesting questions we may ask ourselves are: Will the orbit described by the north end, say, of the needle, be a closed curve or approximately so? That is, will the needle at the end of a certain period assume the same direction that it had before, and again sweep over the same curve in the same length of time? Or, will the needle never return to a previous position, and thus never fulfil a secular variation period? If we have such a thing as a true period, is it the same all over the globe? If we have to deal with *different* periods, as the discussions of declination observations at various stations would seem to indicate, are these local or independent, and thus belong to different systems of magnetic forces? Or, do they but indicate different *stages* in the development of the secular variation, whereby either the period itself is a fluctuating one, or the orbit consists of several branches or loops? If the secular waves travel from east to west, traversing the whole globe, then by making an instantaneous circuit of the earth in an easterly direction, shall we find the needle at every station farther along in its secular orbit? Shall we find a continuously progressive and consistent motion throughout our survey, thus correlating the stations and referring the cause to a common origin? If we find this to be but roughly so, then by selecting as a base station, one where we have a long series of observations, we may with the aid of the shorter series at other stations, by adopting a time-coefficient determined from a comparison of the curves, attempt to answer some of the questions propounded without waiting until centuries have given us a complete period? Finally, what is the law of force acting upon the needle to cause it to describe its orbit?

To carry on a study of the secular variation to the best advantage, it would be highly desirable that at all stations where we have a tolerably long series of observations they be put in the best shape possible by one familiar with the subject and the station. It would then be an easy matter to establish secular variation stations all over the globe, where future observations might be made. This would mean simply the inauguration of a grand scheme, the fruits of which might not be seen for centuries. While such a gathering of material has already been made for many stations, there is, however, abundant material left.

The first station selected for discussion is London, where we have the best series of observations of both elements. The declinations date with Boroughs's in 1580. They can be represented (within their probable error) by the following formula, derived

[1] Abstract of a paper read before Section A of the A.A.A.S., Aug. 18, 1892.

[2] See Bulletins de l'académie royale de sciences etc. de Bruxelles, 47me année, 2me serie, T. xiv.

[3] See U. S. Coast and Geodetic Survey Report for 1885, App. No. 6, p. 272.

from a least-square adjustment of all the observations between 1580 and 1890: —

$$D = +6.24^\circ + 17.75^\circ \sin [0.7^\circ (t - 1850) + 112.7^\circ] \text{ - - - - (1).}$$
$$\pm .10 \quad \pm .31 \quad \pm .3$$

Where D stands for the declination at any time t, positive when west, and $0.7^\circ = \frac{360^\circ}{514}$ = angular motion in one year. The differences between the observed and computed values would seem to indicate a fluctuating smaller period of about 80 years, having a variable parameter of about ½°. Somewhat similar results were found at Paris by Mr. Schott, and at Christiania by Professor Weyer. No attempt was made at present to establish this second term, it lying within the probable error, which for a computed result is ± 20′.

The inclinations date with Norman's in 1576. The observations between this date and 1891 can be represented by the following formula: —

$$I = 70.40^\circ - 3.98^\circ \sin [0.7^\circ (t - 1850) + 23.0^\circ] \text{ - - - - (2).}$$
$$\pm .065 \quad \pm .09 \quad \pm .1$$

Where I = inclination at any time t and the period involved, 514 years — apparently the same as for (1).

The probable error of a computed value is ± 10′, which, considering the material, is satisfactory. Both (1) and (2) apply to latitude 51° 30′ and longitude 0° 07′ west of Greenwich. The mean of the Greenwich and Kew observations was taken to apply to this station. Comparing (1) and (2), a remarkable result peculiar to this station will be noticed — that the epochs are practically complementary, hence the following approximate relation between the declination and dip can be found: —

$$\frac{\delta^2}{(17.75)^2} + \frac{\iota^2}{(3.98)^2} = 1 \text{ - - - - (3).}$$

Where $\delta = D - 6.24$ and $\iota = I - 70.40$.

From (1) and (2) the following results are obtained: —

	Declination.		Inclination.	
	Amount.	Time.	Amount.	Time.
Maximum	24.0° W.	1817.5	74.4°	1688.5
Minimum	11.5° E.	1560.4	66.4°	1945.7
Range	35.5°	— —	8.0°	— —
Mean	6.2° W.	1689 } 1946 }	70.4°	1560 } 1817 }
Zero	0.0°	1660 } 1976 }		

From which it appears that for London the mean declination takes place about the epoch of maximum and minimum inclination, and *vice versa*.

With the aid of formula (1) and (2) the curve described in space by the north end of a free magnetic needle was now accurately constructed and graphically exhibited. It was shown that the first approximation of the curve could be taken as a spherical ellipse, the period being about 500 years. For Paris, a similar result was obtained and exhibited, using provisional formulæ. For both stations the curve lay to the greater part west of the true meridian, and the direction of the motion (standing at the centre of the needle and looking towards the north end) was that of the hands of a watch, or opposite to that of the earth. A rough survey was then made of the globe in an easterly direction approximately in the latitude of London, and elucidated by a diagram. It was found that the needle was farther along in its secular orbit at every station. The curves for some stations in the southern hemisphere were also exhibited.

The following conclusions were reached: —

1. The direction of the secular motion of the north end of a free magnetic needle in both hemispheres is that of the hands of a watch.

2. That if the secular orbit is a *single closed curve*, then are the periods *different* for different stations.

3. That if the period is a *common* one, then must the orbit be a closed curve of two or more loops lying within each other. We are getting then, at present, a small loop in America and a larger one in Europe.

4. That our present feeling is rather that, strictly speaking, we have no such thing as a *period* of secular variation, but that the needle partakes of a sort of spiral motion, returning approximately to a former position, but never exactly so.

Future study may possibly modify some of these conclusions. The possibilities opened up by such a study as outlined were next briefly alluded to, and reference made to a possible extension of the well-known Gaussian analysis by the introduction of the variable t — time.

In conclusion, can we not say with Sabine: "Viewed in itself and its various relations the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit."

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

On Biological Nomenclature.

I HAVE read with interested attention the discussions by botanists in *Science* on this subject. It would appear that they are fully alive to the need of some canons of nomenclature in their branch of biology — a need which has been felt, and supplied of late years, by the zoölogists.

The nomenclature of botany has always seemed to me to be more stable and uniform than that of zoölogy, for the reason, as I supposed, that the naming of new genera and species has, for the most part, been reserved to a comparatively few leaders in the science; and the same cause has contributed to the fixity of botanical classification, in comparison with the incessant taxonomic fluctuations which zoölogy has suffered.

With the late great increase in the number of working botanists, the distinction of a small select "caste" of authoritative namers and describers in botany seems to be breaking down, with the various good and evil results attending this transfer of power from a privileged oligarchy to more democratic rule.

I think that not improbably the botanists who are now exercised over names may examine with much confidence the canons of nomenclature lately formulated and rigidly enforced by the American Ornithologists' Union. These rules have been found to work admirably in practice. They may not be the best possible, but on the whole they are the best extant. A number of leaders in other departments of zoölogy, besides ornithology, as in mammalogy, herpetolcgy, ichthyology, malacology, entomology, etc., have found them entirely available. If some two or three of the rules are not so acceptable as the rest, yet it seems to be generally conceded that it is better to abide by them all, than to dissent from the code as a whole on account of a few comparatively unimportant points that may not be liked so well as the rest are.

Referring to the excellent article of C. H. Tyler Townsend in *Science* of Sept. 16, it seems to me that the moot points he raises have all been considered carefully by the ornithologists, who have settled each of these questions to very general satisfaction; and that the considerations upon which their conclusions have been reached are entirely applicable to the botanical questions involved.

I wish to say a word respecting the somewhat epigrammatic rule, "once a synonym, always a synonym," for the form of which I am measurably responsible, if I remember rightly. Like any other curt sententious saying, the rule is, as I perceive by Mr. Townsend's remarks, liable to be misunderstood. There is no question that, as he correctly says, "If a form which had been described and then thought to be the same as some other species, is later proven to be a valid species, the name originally proposed should stand." Certainly it should. That is not the application at all of the phrase "once a synonym, always a synonym;" and I never heard before of its application to the case Mr. Townsend adduces. What the aphorism really means is best illustrated by a concrete example:

Let there be a genus *Smithia* in botany. Let a genus *Jonesia*

then be named. Let *Jonesia* then be found to be the same genus as *Smithia*. Then the name *Jonesia* "lapses into synonymy" and cannot be thereafter applied *to any other genus* in botany. That is all that is meant by the saying "once a synonym, always a synonym." In other words, if *Jonesia* is not good for what it originally meant, it is good for nothing; it is to be deleted absolutely, and cannot come into re-existence by transfer to any other genus.

Exactly the same principle holds for all specific names within their respective genera. Example: Let there be a *Rosa Smithi*. Let some one then name a *Rosa Jonesi*. Let *R. Jonesi* be considered to be the same species as *R. Smithi*. Then there can never be a *R. Jonesi;* that is to say, *no other species* of *Rosa* can be specified as *Jonesi*. But, of course, if anyone discovers, after this reduction of *Jonesi* to a synonym of *Smithi*, that what had been called *R. Jonesi* is a good species, then *Jonesi* revives as the name of *that* species; and the fact that it had been (erroneously) regarded as a synonym of *Smithi* is no bar to its use in its original sense.

So the expression, "once a synonym, always a synonym," is seen to hold perfectly good in its proper acceptation The fact that a certain name has ever been wrongly regarded as a synonym does not make it a synonym; for it ceases to be such the moment the mistake is detected and corrected, and therefore is not amenable to the rule at all.

I think that, on this reconsideration of the subject, Mr. Townsend may be himself the first to affirm the validity of the now famous maxim, and I am sure that, if he does so, he will find it works well. ELLIOTT COUES.

Smithsonian Institution, Washington, Oct. 10.

Crayfish Attacked by Leeches.

WHILE walking on the beach at Lake Chautauqua one day, recently, I observed a crayfish about four inches in length lying just at the edge of the water, where it had apparently been thrown up by the waves.

On picking it up, I found that it had moulted but a short time previously, and that its new shell was still quite soft. As I lifted the animal, I was surprised to see five large leeches, the smallest of which in its semi-contracted position extended nearly three inches, hanging from the body, and upon a closer investigation observed that all five were attached to a single portion — the left chela. The part which had been attacked by the leeches was the area of attachment of the adductor muscle; and, if the work had not been interrupted by my examination, it would have resulted in the complete crippling of the pinching apparatus of that side. Other and seemingly less protected portions of the body were uninjured.

It would be interesting to ascertain whether the point of attack in this case was accidental or determined by intelligence, but the appearance was that the leeches, appreciating that their prey was just at this time incapable of protecting itself, had deliberately attacked the animal in such a way as to prevent it from protecting itself in case its shell should sufficiently harden before they had succeeded in killing it. The right chela had one slight perforation in it, in the same location, and it is possible that a leech had begun there also, but dropped off unnoticed when the crayfish was raised from the ground. I should be glad to learn of any other observations on the way in which leeches attack their prey.

H. T. FERNALD.

State College, Centre County, Pa., Sept. 27.

A Wasp Study.

NEAR my summer home we have large numbers of the small solitary wasp (*Eumenes smithii*). The mother-wasp digs a passage and cell, usually in the open sandy pathway: our pathways show hundreds of these wasp-holes, about one-half an inch in diameter, while the work is going on and before the cells are closed. The egg having been laid, the mother-wasp provides a caterpillar or two, which she leaves in the nest in a state of coma or paralysis; this coma lasts until the young wasp is hatched, when it finds fresh living food ready for it.

About the time when the Eumenidæ are busy with home-building and egg-laying, we usually have on our wild cherry-trees and young poplars large numbers of the nests of small caterpillars. This year I noticed a remarkable absence of caterpillars; scarcely a web-nest was to be seen. It did not occur to me to wonder what food Madame Eumenes would provide for her babes in this famine of caterpillars, until one day I was treated to a curious spectacle. I saw a slender blue-black wasp about an inch long, carrying off a large gray grasshopper. The grasshopper was fully two inches long, large and heavy in proportion to its length, a handsome insect of a greenish-gray, with some pale yellow touches and markings.

The wasp lay upon the caterpillar, its thorax upon the thorax of the caterpillar, and its sharp-pointed black head resting exactly between the large, full eyes of the captive. The small fore-legs of the wasp clasped the upper part of the caterpillar's thorax; the wasp's third pair of legs lay along the thighs of the large hind-legs of the grasshopper. Clasping its prize firmly with its first and second pairs of legs the Eumenes flew, carrying the grasshopper. Each flight was short, not over from five to ten yards; then the wasp settled, and before flying again made some little progress between walking and flying, dragging the grasshopper beneath it in the position just described. The course of the wasp was in a direct line. It followed the path for a time, but where the path curved or deflected the wasp moved directly over bushes, stubble, and long grass.

Meanwhile, the grasshopper was absolutely quiescent, and had I not known the wasp's penchant for living prey, I should have believed it to be dead. I followed captor and prisoner for some fifty yards, and then seized them both. The wasp clung to her prey, and seemed so absorbed in that as to be heedless of capture. I took the two home in my hand, holding by the body of the grasshopper, put them on a plate under a goblet, and proceeded to examine the case.

The wasp was not biting or stinging the grasshopper, but merely held it firmly clasped, the rigidity of the heads of both insects being very noticeable. The extremity of the abdomen of the wasp trembled slightly; the eyes of the grasshopper had a very peculiar, dull, unseeing expression, like those of a person in a state of coma; occasionally the grasshopper's large thighs quivered, and constantly there was a slow expansion and contraction of the abdominal rings. Finally I forcibly removed the wasp from the back of the grasshopper, and placed the latter on the floor in a draught of air. In a moment or two it seemed to recover itself slightly, stretched all its legs, and gave a feeble hop. I then set the wasp free within a foot of the grasshopper, and seeming to recognize its booty, it dashed upon it, and took the same position as before. There was no biting of the head of the grasshopper. I watched both closely. After this second capture the grasshopper rapidly succumbed: its first pair of legs curled up closely; the second pair folded together into a kneeling posture; the hind-legs were extended, quivered no longer, and the abdominal expansion and contraction were feebler and slower.

At the end of twenty minutes I removed the wasp, carried her to the other side of the house and set her free. She departed as if reluctantly "enforced to go and seeming still unready," evidently all her mental powers, whatever they might be, were concentrated upon that grasshopper. I returned to the grasshopper, and found it giving no signs of life except the abdominal motion. I then sprinkled it thoroughly with ice-water. It recovered a little, moved its thighs several times, but the contraction of the first and second pairs of legs, and the motionless, stiffened state of the antennæ, were very marked. In whatever position I put the insect there it remained, on back or side, or propped up on its bent, "kneeling" fore-legs. The slight reviving produced by air, water, and freedom from its enemy did not last. The grasshopper grew more rigid and the ring expansion less and less marked. I desired to keep the creature to discover how long it remained uncorrupted in this state, but a sharp gust of wind blew it from my hand into grasses where I could not recover it.

The manner of the carrying of the grasshopper by the wasp, the strength exhibited by the wasp, its absorption in its prize, the peculiar resting of its head motionless upon the upper portion of

the grasshopper's head, and the singular state to which the grasshopper was reduced, all seemed to me worthy of notice.

Evidently the grasshopper was carried off to serve for the food of the young larva, instead of the caterpillar usually provided. Had the grasshopper been paralyzed by a sting or bite? Was it mesmerized or hypnotized by its vigorous little enemy? Whatever had been done to it, it was absolutely quiescent and making no manner of fight for itself. JULIA MCNAIR WRIGHT.

Fulton, Missouri.

Auroras versus Thunder-Storms.

DURING September just past sun-spots were very numerous and large. Nevertheless, auroras during the month were without exception comparatively inconspicuous. In this case certainly large sun-spots have not been attended by bright auroras, as some have held to be the rule. The explanation of this anomaly, which appears to be justified by systematic records in my possession, is that thunder storms took the place of auroras. It has been found that not unfrequently thunder-storms become widely prevalent upon dates upon which auroras should fall in accordance with their periodicity corresponding to the time of a synodic revolution of the sun. When this happens, it robs them of their brightness, wholly or in part. The relation between these two classes of phenomena appears to be reciprocal or substitutive, the one taking the place of the other under conditions which are only just beginning to be understood, and which are in process of investigation. M. A. VEEDER.

Lyons, N.Y., Oct. 8.

European Origin of the Aryans.

IN reference to Dr. Brinton's note in *Science*, Sept. 16, I certainly have not read all D'Halloy's writings, which seem to me to have no present scientific value, possessing merely a faint historical interest. I only professed to have read those passages which Dr. Brinton cited in his lectures.

The extract which Dr. Brinton now gives from the article of 1843 only confirms me in my conclusions. D'Halloy's mention of an Himalayan origin, and his allusion to the hypothesis that the Indo-Germanic languages were derived from Sanscrit, point rather to an acquaintance with Adelung's Cashmere theory of 1806 than to any adequate knowledge of the Central-Asian hypothesis of Pott, Lassen, and Grimm, which dates from 1847–48. At the time when D'Halloy, in his "Éléments d'Ethnographie," appended a note to this article, he must have heard of the Central-Asian theory; but the "Éléments d'Ethnographie" I had not looked at, as it was not one of the works cited by Dr. Brinton.

However, the matter is so unimportant that if Dr. Brinton still wishes to maintain his view, we may agree to differ.

ISAAC TAYLOR.

Settrington, York, England, Sept. 29.

Change of Diet in Birds.

EVERYONE who has a garden must have noticed the manner in which the common sparrow destroys the flowers of the yellow crocus. The earliest mention of this which I can find is in *Science Gossip* for 1865. The question is, Was the bird previously in the habit of thus destroying crocus flowers,—I do not say eating,—or is it a new departure?

Since then I have observed that the common yellow primrose is similarly injured by sparrows. Seeing a crowd of sparrows busy among some primrose plants in my garden, I made a close examination of their work. Some of the flowers had been entirely plucked off; in others the entire cradle and some of the petals had been bitten off and dropped on the ground, but nothing appeared to have been eaten. I examined a number of the flowers carefully, first with the naked eye and then with lenses of different powers, but I could find no traces of insects which the sparrows might be supposed to have been seeking.

The main point is, then, What is the motive of the sparrows in thus singling out the crocus and the primrose for attack?

W. SLATER.

London, England, Sept. 29.

BOOK-REVIEWS.

The Speech of Monkeys. By R. L. GARNER. New York, Chas. L. Webster & Co. 8°. 233 p. $1.

The work of Mr. Garner upon the "Speech of Monkeys" is already well known through the public press, and all who have become interested in this extremely suggestive subject will be pleased that he has summarized in a neat little volume the important results of his work up to the present time. Nearly all of the facts published in this volume have already been given to the public through the pages of the *Cosmopolitan*, *The Forum*, *The North American Review*, and other publications; but in this volume he has brought together all of the important facts given in these various places. Mr. Garner has been at work upon the subject of monkey language for some eight years, and, although a number of interesting facts were seen earlier in his observations, it was the application of the modern phonograph to the study which for the first time put the subject upon a scientific basis.

The present work is divided into two parts. The first part gives an outline of the facts as he has observed them, and his methods of experiment. As one reads this part he receives two impressions. The first is as to the exceeding scantiness of the definite results. It is perhaps a little disappointing to find that the speech of monkeys as thus far made out by Mr. Garner seems to be confined to a few sounds, nearly every one of which has a variety of meanings, or rather does not seem to have any exact significance. This is, after all, not to be wondered at. Mr. Garner himself recognizes that he has only made a beginning in his researches, and it must be remembered that he has had absolutely no guidance from the previous work of others. Moreover, it is to be expected from their general grade of intelligence that the speech of monkeys will be confined to a few ideas, and those ideas of the widest general signification. The second impression that we receive as we read the book is, that Mr. Garner's work, so far as it has gone, is reliable and that he is dealing with facts rather than fancies. We cannot but feel that the few facts which Mr. Garner has made out are well authenticated. It is very striking when we learn that Mr. Garner has so far discovered the speech of monkeys as to have learned the password into their good graces, and we cannot but be interested in his ability to attract the attention of monkeys by saying to them, in their language, the word which means food. His ability to thus obtain their good-will by the use of a word of their own language; the fact that monkeys always use this word in connection with food; the very fact that the meaning of the word is vague, being used in connection with food or drink, or "any kindly office done them;" the fact that other actions of monkeys are also always accompanied by a perfectly definite sound, which Mr. Garner has in many cases been able to imitate; the fact that a repetition of these sounds in all cases will produce similar actions in other monkeys of the same species; the fact that monkeys of different species do not use the same sounds under the same conditions; the fact that occasionally one monkey learns a word used by another species of monkeys for certain purposes; the fact that monkeys do not use these words when alone but only when they have some one to talk to; and many other incidental occurrences combine to give us the feeling that, so far as Mr. Garner has gone, his belief that monkeys have speech is well substantiated.

As one reads this work, he is at some loss to know in his own mind whether to predict that Mr. Garner is going to be able to develop the speech of monkeys to a great extent, and is only on the threshold of important discoveries, or whether he has already nearly reached the limit of their speech. Their language, of course, cannot advance beyond their knowledge, and it may be that their speech will be confined to the vague expression of a few of their crude conceptions of nature. Mr. Garner believes, however, that there is very much to be still discovered, and that the speech of monkeys is of more importance to them in expressing their ideas than their gestures.

The second part of the work is of considerably less interest, being more in the line of speculation. It gives the theoretical deductions which Mr. Garner is inclined to draw from the facts he has already seen, and some few speculations as to the origin

and significance of language. In this part of the work we must find the chief interest in the fact that we have an attempt to theorize as to the origin of speech from the standpoint of its beginning in the lower animals, rather than from the standpoint of its more developed conditions in man. The study of language hitherto has been to reduce human language to its lowest and simplest form. Mr. Garner for the first time attempts to develop language from its simpler conditions in the lower animals, and if Mr. Garner's conclusions differ in some respects from the ideas that have hitherto been in vogue, it is not surprising.

On the whole, the work of Mr. Garner is extremely interesting and suggestive. As a piece of book-making it is open to criticism. It is sketchy; it is not very logically arranged, containing a miscellaneous mixture of observations on the intelligence, habits, gestures, affections, and general mental attributes of monkeys, some of which are new, but most of which are not especially new, and have little relation to the subject of monkey speech. The observations on the actual speech of monkeys, which is of course the really valuable part of the work, fills only a small portion of the book. We must look upon this volume and the work it describes simply as an outline sketch of the beginning of a series of results which may be carried to a successful issue in later years. The thanks of science are certainly due to Mr. Garner for opening to us a new line of research and a new realm of suggestive thought.

H. W. Conn.

Outlines of Zoölogy. By J. Arthur Thomson. Edinburgh and London, Young J. Pentland, 1892. 655 p. Ill.

For some years now there has been no text-book of zoölogy in the English language at all adequately representing the present state of the science, and at the same time of moderate cost. The cost of Claus and Sedgwick is high; the translation of Lang must remain incomplete till the original shall be finished; Lankester's promised book still delays its appearance; Packard is out of date, as for that matter is Claus and Sedgwick; and the college teacher who wishes his students to have a good reference book in their possession hardly knows where to turn when the said students combine a comparative ignorance of German with thinly-lined pocket-books. Under these circumstances, the prospect afforded by the announcement of Mr. Thomson's book was distinctly attractive.

It may be said at the outset that the book to a large extent responds to favorable anticipations. Mr. Thomson, while not much known as an original investigator, has made a record for himself in the hardly, if at all, less useful *rôle* of abstractor and collator of the work of others, while his occasional essays and his work with Professor Geddes on the evolution of sex have shown him to possess an agreeable literary style. The "Outlines of Zoölogy" is an exceedingly readable book, and perhaps the only criticism that can be made upon its style is that it occasionally degenerates into flippancy. Professor Forbes was quite justified in making his joke about the "wink of derision" which *Luidia* gave him as it passed over the side of the boat after breaking off its arm; but it is hardly desirable to waste space in repeating the joke in a text-book. A good many examples of this kind might be quoted.

Mr. Thomson wisely, we think, follows the example of Claus, Boas, and other writers in devoting a considerable amount of space to general matters. The first ninety pages of the book are occupied with an account of the functions of animals, the meaning of organs, tissues, and cells, methods of reproduction, fertilization, segmentation, etc., palæontology, distribution, and the principles of evolution. Evidently these subjects must be treated in the briefest possible way; but the result is on the whole not unsatisfactory. The first chapter, however, which takes for granted a knowledge of the meaning of such words as "cells," for example, would be a pretty tough morsel for the average student beginning zoölogy without any biological training. Of the remaining 514 pages (excluding index), 343 are taken up with invertebrates, and 171 with vertebrates — an arrangement which, for a general text-book, surely gives too much space to the vertebrates.

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Exchanges.

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For Sale or Exchange.—The subscriber would like to receive cash offers, or an offer in exchange for the earlier volumes of Poggendorf's Annalen and the later volumes of Silliman's Journal, upon the following list: Chenu.—Manuel de Conchyliologie. 2 vols. Nearly 5,000 figures, some hand-colored Paper. Paris, 1859. Edwards.—Butterflies of N. A. 2 vols. Plates hand-colored. Vol. I., half calf Vol. II. in parts. Leyman, Agassiz, Hagen.—Ills. Cat. Mus. Comp. Zool. at Harvard. No. I. Ophiuridae. No. II., Acalephae. No. III., Astacidae. All bound in one volume. American Naturalist. Vols. I.–VII. Cloth. Silliman's Am. Jour. of Science and Arts. Third Series. Vols. I.–X. Cloth. Binney.—Terrestrial Mollusks of N. A. Colored plates. 4 vols. Stretch.—Zygaenidae and Bombycidae of N. A. Colored plates. Also a considerable library of monographs, reports, and scientific books, and a large number of duplicates of fossils, minerals and shells. E. A. STRONG, Ypsilanti, Mich., Sept., 1892.

Exchange.—I have the finest shells of Anodonta corpulenta, C'p'r, and Suborbiculata, Say, in the world. Will exchange for fresh water, land, and marine shells, in large or small quantities. Have also for exchange 50 varieties of fresh water and land shells from Spoon River, Ill. Dr. W. S. STRODE, Lewistown, Ill.

To exchange for books on birds or insects, or for back volumes of American Naturalist: Ecker's "Anatomy of the Frog," Packard's "Guide," Guyot's "The Earth and Man," Rockhill's, "The Land of the Lamas," Parker's "Biology," Shoemaker's "Heredity, Health and Personal Beauty," Dexter's "The Kingdoms of Nature," all new. M. J. ELROD, Ill. Wes. Univ., Bloomington, Ill.

For Sale.—About 1087 volumes of the private library of Dr. Nicolas León, formerly director of the Museum at Morelia, embracing publications of special value for Mexicologists, like those of Bishop Zumárraga (16th century), of Siguenza y Gongora, of Aleman, etc., the Missal of Spinoza, all very scarce: manuscripts on the history of Michoacán and other Mexican States, on the Tarasco (the Indian language of Michoacán) and several works, of which the only copy known to exist is in this collection. Parties interested in the sale please address Dr. Nic. León, Portal de Matamoras, Morelia, Mexico.

For Exchange.—"The Birds of Kansas,"—Goss, for Gray's Anatomy, or Medical Dictionary. Must be in good condition. Address, J. H. Simpson, Buchtel College, Akron, Ohio.

Wants.

METALLURGICAL CHEMIST will give instruction in the metallurgy and chemical analysis of iron and steel. Complete or special courses applying to the manufacture of pig irons and steels, as well as to their uses. Address "METALLURGIST," care Science.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hertig, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.

WANTED.—A position as zoological artist in connection with a scientific expedition, institution or individual investigations. Experienced in microscopic and all scientific work. References given if desired. Address J. HENRY BLAKE, 7 Preston Place, N. Cambridge, Mass.

CHEMIST AND ENGINEER, graduate German Polytechnic, Organic and Analytical, desires a position in laboratory or chemical works. Address 213½ E. 7th Street, New York, care Levy.

A MAN, 36 years old, of extensive experience, having the degrees of A.M. and Ph.D., desires a first-class opening as a teacher of Zoölogy and kindred studies in a college or university. Can furnish numerous testimonials or references as to success and skill in most modern laboratory methods. Address E. W. D., Md. Agr. College, College Park, Md.

A JOHNS HOPKINS graduate (1892) desires a position as instructor in mathematics and physics. Address A. B. TURNER, Johns Hopkins University, Baltimore, Md.

A YOUNG MAN, with a thorough training in Analytical Chemistry (including analysis of minerals, food, water, etc.), and holding a diploma of the School of Practical Science, of Toronto, and good testimonials, desires a position as Analytical Chemist or as assistant to such. Address to WM. LAWSON, 16 Washington Ave., Toronto, Ontario.

In treating of each main division of the animal kingdom, the author begins with a classification of the group and general remarks on its biology and characteristics. Then he takes each class in order, and gives an account in some detail of a type-animal, followed by notes on other interesting genera in the class. In the smaller classes, the type may be omitted. Finally the embryology and affinities of the group are discussed. Sometimes the orders also are characterized, especially among the vertebrates, at other times this is unnecessary.

It is impossible to mention fully either the good or bad points of the book in a short review. It is very well up to date in most respects. Résumés are given of important discussions, such as that regarding the origin of the vertebrates, the position of *Balanoglossus* and other Hemichordata, etc. In most cases the treatment of the various subjects discussed is impartial to a degree. We hear something of "anabolism," "katabolism," "maleness," "femaleness," and so forth, but not too much; and the author's opinions are never dogmatically expressed. On the opposite side of the account must be placed the fact that some phylogenetically important groups are very inadequately discussed, apparently because of their small size, the Polyzoa and Brachiopoda, for example. Among the Rotatoria, *Trochosphæra* is apparently not mentioned at all. A feature of questionable advantage is the constantly recurring tables of resemblances and differences between families, orders, classes, and sub-kingdoms. These tables undoubtedly present matters in a striking form, but as undoubtedly they lead to "cram work" in the case of many students. Perhaps this is the reason for their presence. Mr. Thomson says that his book is partly intended for medical students, who, for the M.D. degree in Edinburgh and other British universities, are required to pass an examination in zoölogy. It may be that the tables were inserted with a special view to their requirements. And still another serious defect in the book is the way in which it is mis-illustrated by 32 full-page plates of rough diagrams. None of the illustrations can be called good, some are atrocious.

Still, taking the book all in all, it is perhaps the best lecture-companion for college students in English at present, and it is to be hoped that an American edition may soon be forthcoming.

JOHN GARDINER.

University of Colorado, Boulder, Col.

AMONG THE PUBLISHERS.

To encourage the use of the microscope, which certainly has proved a fascinating hobby for many as well as a most important art for all science-workers, the Bausch & Lomb Optical Company, Rochester, N.Y., has for some time published a hand-book, entitled "Manipulation of the Microscope" (75 cents), by Edward Bausch. That this book has served a good purpose is evidenced by there having been ten thousand copies sold.

— In the announcement of new books and new editions for the holiday season of 1892 the eight-volume set of "The Lives of the Queens of England," by Agnes Strickland, is foremost in J. B. Lippincott Company's list. The work is a reprint of the author's latest revised edition and contains portraits of the queens of England and numerous other illustrations especially prepared for this edition. Dr. Charles C. Abbott, so well known as a most delightful writer upon nature, has a volume of his "Recent Rambles." Most of them were made through the Delaware Valley, the region he has made peculiarly his own, but by way of contrast we also have trips in New Mexico and Arizona, and a wandering through a seaside forest. This is the first one of Dr. Abbott's books to be illustrated, and it contains nineteen reproductions of photographs made by himself. In addition to other important announcements, we note that Mr. W. S. Baker has again laid students of American history under obligations in the "Itinerary of General Washington," which with great pains and accuracy he has compiled from original sources. The work embraces the period of the revolutionary war.

CALENDAR OF SOCIETIES.

Numismatic and Antiquarian Society, Philadelphia.

Oct. 6.—The president, Dr. D. G. Brinton, described a recent visit to the aboriginal jasper quarries in the Lehigh Hills, Pennsylvania. Two sites were examined, in company with the discoverer, Mr. H. C. Mercer, and Mr. Charlemagne Tower, president of the Board of Managers of the Museum of the University of Pennsylvania. The pits at one site numbered about sixty; at the other nearly double that number. Some were eighty feet in length by twenty to thirty feet wide and must have been at least twenty to thirty feet deep. An enormous amount of the material had been excavated and carried away to be worked into arrow and spear-heads. Quantities of quarry rejects were visible, and one work-shop site was visited. The discovery of these quarries adds greatly to the correct knowledge of the archæology of Pennsylvania. Dr. Robert H. Lamborn exhibited and described two copper turtles of singular workmanship, one from the Casas Grandes, the other from Chiriqui, Central America. Both displayed the peculiar character of wire-work in use by the native copper and gold-smiths.

To the Readers of SCIENCE.

During the past year it has been found possible to enlist the interest of scientific workers in the success of *Science* to such an extent that more than eight hundred have promised contributions during the coming twelve months. Not only are contributions of merit coming in ever increasing numbers from American scientific men and women, but we are now securing our first contributions, in any quantity, from abroad.

We know that this development in the usefulness of *Science* is appreciated, not only from the many kind letters received, which are always inspiring and which we wish our friends would mark as at our disposal for publication, but from a marked increase in the number of new subscribers.

Science owes its existence to the munificence of two gentlemen, whose names we do not feel at liberty to publish, who contributed very nearly $100,000 toward the support of the paper in its early years. There is no longer need of such liberal subsidizing, but we do need cash subscriptions from all who feel at all interested in a weekly journal of science in America.

There is no question that scientists are cosmopolites and that a journal is the more useful to them the more it is international in its character. As the result of our efforts to develop the use of *Science* abroad, we have recently published articles from V. Ball, Dublin; Edward T. Dixon, Cambridge, England; A. H. Keane, London; David MacRitchie, Edinburgh; Edward Seler, Berlin; Isaac Taylor, York; G. D. Liveing, Cambridge, England; the Marquis de Nadaillac, Paris; Miss Agnes Crane, Brighton, England; E. Trouessart, Paris; J. Edmund Clark, York; and have in hand for early publication a number of papers from prominent European scientific men.

To develop this international feature of the paper an enlargement to twice its present size will be necessary, and an increase of the price to six dollars.

If we can secure a sufficient increase in the number of subscribers we can promptly enlarge and improve *Science* still further, but cannot without, as in the past the work, as will be seen, has been largely a labor of love. Within the acquaintance of each of our readers there must be some one sufficiently interested in the development of what we hope is a valuable means of scientific discussion to subscribe six dollars, and we urge each and all our friends to do what they can to help. If the number of new subscribers is as large as we hope, one-half of the enlarged *Science* would be printed and published in London to facilitate promptness of publication.

Form of Subscription.

N. D. C. Hodges, 874 Broadway, New York:

Enclosed is check (money order, or whatever it may be) for six dollars, for which enter me as a subscriber to *Science* for one year and thirty-seven weeks, it being understood, however, that if the number of new subscribers received justifies the enlargement of the paper to twice its present size, the price being raised to six dollars per year, the term of my subscription shall be curtailed *pro rata* for the unexpired term.

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